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A Note on the Benefits of Homeownership

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I. A Note on the Benefits of Homeownership

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Abstract

This brief note adds to recent work that attempts to identify externalities associated with homeownership. The results suggest that some of the homeownership effect found in Green and White [5] is driven by family characteristics associated with homeownership, especially residential stability. However, as much as homeownership increases residential stability, it appears to be correlated with higher school attainment. Attempts to control for endogeneity cannot eliminate this finding.

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Introduction

A number of recent studies attempt to measure whether there are nontraditional benefits to homeownership, such as increases in the success of children (Green and White [5]), citizenship (DiPasquale and Glaeser [3]), and a variety of family outcomes and attitudes (Rossi and Weber [11]). Because of the preferential tax treatment accorded homeowners, particularly low-income homeowners, and the large degree of wealth accumulated in housing, these authors argue that it is important to know the full range of homeownership benefits and costs. However, given the difficulty of credibly assigning causality to housing externalities, it is not surprising that such factors have been previously ignored.

In one such paper, Green and White [5] find a strong statistical correlation between homeownership and the likelihood of dropping out of school or becoming pregnant. Yet a reasonable interpretation of their result is that of omitted variable bias. Clearly, homeowners are different from renters along a variety of dimensions. As a result, those factors that are latent in their work, such as parental skills, interest in the educational process, wealth, and family stability, potentially bias upward any homeownership effect. While the authors claim that their results are robust to parametric self-selection corrections, these techniques require assumptions about the selection equation that are difficult to defend.

However, beyond pure selection, there are several mechanisms that suggest that this could be a causal relationship. Most plausibly, homeowners have a large financial stake in their community and therefore may invest more in neighborhood and school capital. Epple and Romer [4] and DiPasquale and Glaeser [3] have modeled the different incentives faced by owners and renters. For our purposes, the key insight is that landlords recoup any community-specific investment that is made by renters, while homeowners are able to internalize the future returns to these investments because they accrue as increases in the value of their home. Therefore, homeowners have a much stronger incentive to participate in the growth of neighborhood capital. In fact, DiPasquale and Glaeser find that homeownership has a causal effect on community investment. As investment in a community grows, it is possible that children's educational outcomes will improve, perhaps providing the missing link in the neighborhood effects literature. On the other hand, time and money committed to neighborhood and housing investment might be offset by reduced input into family-specific investments that have a more direct payoff to children's outcomes. For example, Currie and Yelowitz [2] argue that public housing has a positive effect on school retention because subsidized housing allows money to be directed to other family needs.

An alternative but related mechanism works through family residential stability. Several recent papers, including Hanushek, Kain and Rivkin [6], McLanahan and Sandefur [8], Astone and McLanahan [1], and Haveman, Wolfe, and Spaulding [7], explore the impact of family or school mobility on student achievement. They argue that residential mobility might be causal if it leads to a loss of social capital in the form of less information and attachment to the school system, teachers, and peers.[1] The most compelling evidence is presented in Hanushek, Kain, and Rivkin, who, using individual fixed effect models, find that residential or school moving has a significant negative impact on student achievement, particularly for minorities, low income families and students in schools with high turnover. Thus the homeownership effect may be the result of additional family and school stability offered to students who do not have to switch schools or peer groups.

This brief note uses a number of methods to determine whether homeownership is a statistically meaningful factor in predicting children's educational outcomes. No structural estimates of children's attainment are presented, and therefore these results are susceptible to criticism about the causal nature of this relationship. Instead, the paper augments the work of Green and White by estimating more detailed reduced form specifications of the homeownership effect. I

[1] But there may be costs to homeownership if owners are less able to move in response to income shocks (Oswald [9]).

employ a fuller set of background variables that are plausibly correlated with homeownership and the error term in an educational attainment regression. Furthermore, instrumental variable estimates are presented that attempt to solve problems associated homeownership and mobility endogeneity.

The results suggest that the homeownership effect is fairly robust in a simple regression framework, although some of the effect is likely due to difficult-to-measure family characteristics and much of the remaining effect works through its impact on lowering the probability of moving. Like Hanushek, Kain, and Rivkin, these effects appear to be concentrated among low-income households and neighborhoods with lower mobility rates.

Data

This analysis uses the Panel Study of Income Dynamics (PSID) and its accompanying geocode database. All children that reach the age of 17 between 1975 and 1993 are included in the sample so long as they are observed for at least six years between ages 7 and 16. This latter restriction allows a full description of the family and neighborhood background experienced by the child. The dependent variable is an indicator of whether the individual graduated from high school by age 19. The standard individual and family control variables are included in all of the regressions to follow. These include the race and gender of the child, whether the child is in a stepfamily, the parents' education, family income, whether the head worked in the last year, family composition, and the number of kids in the family. Time dummies are included to capture secular trends in schooling. The time varying variables are averaged over ages 7 to 16 to more fully capture the history of the child's background (Wolfe et. al. [13]). Additional variables that are used in the analysis include the residential mobility of the family, home equity and value, and various asset measures from the 1984 and 1989 wealth supplements.

The PSID analysis is augmented by information from the geocode database, which includes detailed geographic information on residential location in zip codes, census tracts, and enumeration districts from 1968 to 1985. These locations are linked to population and housing characteristics from the 1970 and 1980 census. Therefore, this dataset allows construction of duration measures related to time in the community, as well as the mobility of neighbors.

Results

Some of the partial derivatives from probit regressions similar in spirit to Green and White are presented in table 1. Full regression results are available upon request. Some differences between our specifications make direct comparison difficult. The most important difference is in our choice of dependent variable. I use a discrete variable that equals one if the child graduates from high school by age 19, while Green and White use a discrete variable that equals one if the child is still in school at age 17. My sample encompasses kids who turn 17 between 1975 and 1993, while Green and White concentrate on the 1980 to 1987 period. Finally, as already noted, homeownership, family composition, family income, and head's work status are averaged over ages 7 to 16, while their variables are measured at age 17.

Although some of our coefficients differ (not shown), the homeownership derivative is of comparable magnitude and significance. In a simple probit framework, the marginal impact of living in owner occupied housing on the probability of high school graduation is 9.6 (1.5) percent for a base case child who is white and male, and lives in a household with married parents, two siblings, average income, and a head that is a high school graduate.[2] This is displayed in column 1.

<u>The Role of Observable Family Characteristics</u>

While the regression includes gender, race, family status, parents' age, head's education, family size, family composition, marital status, and work status controls, other confounding factors

[2] Using the 'in school by age 17' variable does not change any of the results noticeably. If anything, the findings are smaller but in-line with those reported by Green and White. Using a homeownership indicator measured at age 17, like Green and White, the marginal effect is 7.7 percent.

may be driving this large homeownership finding. In particular, four possibilities are explored: family status changes, parental involvement, residential stability, and wealth.

Perhaps the most obvious explanation for the homeownership effect is the impact of family stability and involvement, both factors that may be correlated with homeownership and children's educational attainment. While the PSID is somewhat limiting in its ability to measure parental involvement, it thoroughly documents changes in household marital, work, and family composition history. When detailed indicators of the timing and frequency of marital, work, and family size changes are included, they have little impact on the homeownership estimates.[3]

To explore the impact of parental involvement, I matched the National Longitudinal Surveys' older men and older women files, which include home ownership questions, to the younger men and younger women files to take advantage of the unique family information reported in these surveys. In particular, there is information on IQ scores, PTA involvement, and whether a newspaper and library card is in the house at age 14. The homeownership effect is similar to that in the PSID and none of the additional variables affect its magnitude.

Third, I include measures of the frequency of residential moves and the duration of residential and neighborhood residence. After controlling for the fraction of years moved between ages 7 and 16, almost one half of the homeownership effect disappears, as shown in column 2. Columns 3 and 4 explore the possibility that the distance of the move is important by measuring the additional impact of switching zip codes or states. The results in column 3 suggest that there is no additional impact of moving across zip code relative to within zip code. However, long distance changes, as represented by across state moves have a positive and large effect, enough to suggest no statistically significant mobility effect for those children who cross state borders. This result

[3] This is consistent with Haveman, Wolfe, and Spaulding [7], who find little difference in their mobility point estimates when a host of family and individual controls are added to a high school graduation regression equation.

suggests that unobservable differences, such as the reason for a move, between across state and within state movers are important. Further evidence on this point is given below. Column 5 shows that longer residential duration has an additional positive impact on educational attainment, even after conditioning on mobility frequency. Finally, column 6 reports mobility effects separately for homeowners and renters. Homeowner family mobility is calculated in the "% years moved as homeowner" variable. Mobility for families that rent are captured in the "% years moved as renter" variable. For families that switch housing status, a "% years moved as homeowner" variable is computed for the years that they were homeowners and a "% years moved as renter" variable is calculated for their renter years. This categorization reveals that mobility hurts both homeowners' and renters' children. Furthermore, a t-test of the difference between the homeowner and renter coefficients cannot reject that they are the same.

However, while I include a variety of family background controls in the basic regressions, it is reasonable to interpret part of the large residential mobility effect to be due to latent characteristics, especially in light of the state mobility findings. One hint that this is so comes from the self-reported reasons for residential moves. The PSID asks household heads the primary reason for a residential change. Possible answers are job-related (to get nearer to work, to take another job, or because of a transfer), consumption-related (housing expansion/contraction, more/less rent, want to own home), neighborhood-related (better neighborhood or school, closer to friends/relatives), or outside events (evicted, armed services, health reasons, divorce, retiring because of health). A small fraction of respondents give ambiguous or mixed reasons. This response is lumped into the outside events category. Decomposing mobility factors by reason, the largest impact of mobility on children's education is from other/mixed reasons and the smallest from job reasons, although even job related moves have a negative impact on the likelihood of high school graduation. Nevertheless, the large other/mixed reason effect suggests that unobservables may still play a key role in the correlation between homeownership and mobility, despite the reasonably long list of control variables. For example, if the move variable counts only the other/mixed responses, the homeownership effect declines from 9.6 to 7.8 percent. Therefore, latent family stability factors explain *at least* a fifth of the homeownership effect. Including all reasons reduces the homeownership effect another 3 percentage points. Therefore, an additional two-fifths of the homeownership effect could be from lowering the probability of residential changes.

A fourth explanation for the homeownership effect is that, all else equal, homeowners are wealthier and therefore can afford better schooling opportunities for their children. Time-averaged family income and the head's educational attainment capture part of this wealth effect. Nevertheless, table 2 reports the homeownership and mobility partial derivatives when additional controls for home equity and other assets and debts are added to the regression specification. The results tentatively suggest that part of the homeownership effect is due to higher levels of home equity. That is, homeownership has a larger impact on children's outcomes for those with home equity at the top of the distribution. Depending on which home equity measure is employed, the difference in high school graduation likelihood between households at the 90th and 10th percentile of home equity is 4 to 4.5 percentage points.[4]

The home equity effect might arise for two reasons. First, households with bigger stakes in their home may invest more in their community. Second, the home equity differences may simply be wealth differences that are correlated with other family characteristics that affect children's well being. It is difficult to decompose the wealth and consumption aspects of housing. But detailed wealth information of some of the households is available during the second half of the sample period. In table 2, I use the 1984 and 1989 wealth supplement of the PSID to estimate whether other asset measures are correlated with children's educational outcomes. The sample is restricted to children who turn 17 between 1984 and 1993. All children who turn 17 between 1985 and 1989 are set to their households' 1984 assets and all children who turn 17 between 1990 and 1993 to their household's 1989 assets. These results suggest that the only asset measures adding explanatory power are housing, and to a much less extent vehicle, equity. Controls for debt, other real estate, cash, stocks and bonds, and pensions (not shown), are insignificant in high school graduation equations. This suggests that the home equity effect may be more than a wealth effect.

Does Neighborhood Stability Matter?

An interesting test of the mobility-homeownership effect is whether neighbor mobility matters. If this is a story about peer or school stability, the residential stability of neighbors could be important as well, all else equal. Table 3 stratifies the sample by the fraction of the neighborhood that has lived in the same residence during the previous five years. In columns 1 to 3, the sample includes those children who grew up in communities with 'neighborhood stability' below the median (58 percent), and columns 4 to 6 report analogous results for children who grew up in neighborhoods above the median. Looking first at the highly mobile communities, the 6.2 (2.0) percent homeownership effect that is reported in column 1 disappears when controlling for household residential duration measures in columns 2 and 3. However, the low mobility neighborhoods show a much stronger and robust homeownership effect, even after controlling for duration. Wald statistics reported at the bottom of the table suggest that the homeownership and mobility effects are stronger in low mobility communities, consistent with the notion that changes in peer groups and stable environments positively impact the educational outcomes of children.

Certainly, this neighborhood stability effect could be confounding other characteristics, such as school quality and neighborhood wealth. But table 4 shows that the neighborhood mobility result does not appear to be a neighborhood income effect. Without controls for mobility, the graduation

[4] Column 2 uses the house equity measure that is asked annually in the PSID. Columns 3 to 8 use asset information

effect of growing up in an owner-occupied home is approximately 12 percent (0.038) in a low income (bottom quintile) community but only 4.2 percent (2.4) in a high income (top quintile) community. These point estimates are significantly different at the 10 percent level. Controlling for mobility and residential duration measures eliminates the homeownership effect in high income neighborhoods but not low income neighborhoods. Although the mobility and duration measures eliminate over 40 percent of the homeownership effect in low-income communities, the homeownership effect of 6.7 (4.0) percent is still significant at the 10 percent level. Furthermore, the -0.238 (0.094) point estimate of the mobility effect for low income neighborhoods is quite a bit stronger than the –0.100 (0.053) point estimate in high income neighborhoods, although because of a lack of precision of the estimates, we cannot reject the null that the estimates are equal. Finally, column 6 shows that most of the mobility effect in high income neighborhoods is due to the other reasons category, suggesting that unobservable family changes are driving any mobility/homeowner effect for the high income communities. Therefore, we conclude that the stable neighborhood result is not a proxy for high income, and thus probably not high school quality, communities.

Instrumental Variable Results

An obvious concern with the probit results is that homeownership and residential mobility are endogenous. Both characteristics are likely correlated with latent measures of children's educational attainment. However, finding a valid instrument is difficult. To deal with the endogeneity of homeownership, I use the strategy adopted in DiPasquale and Glaeser [3]. These authors employ group average homeownership rates as instruments. The homeownership rates are formed by taking state-year average homeownership rates by race and income quintile using the March CPS surveys.[5] The idea is that average homeownership rates may pick up regional variation

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from the 1984 and 1989 wealth supplements.

[5] The CPS is used instead of the PSID for sample size reasons. However, homeownership questions began in the March CPS in 1977, so the 1968 to 1976 rates are held constant at the 1977 level.

that is driven by housing costs, property tax rates, interest rates, and other secular trends in housing. Differences in housing rates across regions and income-race groups should differ in ways that are unrelated to the unobservable components of children's' educational outcomes. As an instrument for residential mobility between ages 7 and 16, I use family mobility rates prior to the child turning age 5. These pre-school moves are likely to predict future recurrences of family mobility but may not impact the child's school progress. As far as these assumptions are incorrect, the instruments are invalid. Statistics on the relevance of these instruments, along with the results, are reported in table 5.[6] Two regressions, one that includes homeownership and one that includes homeownership and residential mobility, are shown for five samples: the full, the low and high income neighborhood, and the low and high mobility neighborhood samples. Estimates are calculated using Rivers-Vuong [10] two step maximum likelihood procedure with bootstrapped standard errors.

As expected, the estimates are smaller than the simple probit and, with only one exception, are statistically insignificant after controlling for residential mobility. For example, among the full sample of children, the two-stage estimate reduces the effect of homeownership from 9.6 (1.5) percent in the simple probit case reported in table 1 to 7.1 (1.9) percent. Including the mobility measure reduces the homeownership effect to an insignificant 3.6 (2.6) percent. However, even this estimate cannot be statistically distinguished from the simple probit estimate of 5.4 (1.6) percent. Likewise, among the low and high income neighborhood and high mobility neighborhood samples, the two-stage estimates reduce the estimated homeownership effect slightly, but after controlling for

[6] Table 5 reports partial R^2 and F-statistics of the instruments from the first stage regressions, as well as Davidson-MacKinnon χ^2 statistics from reduced form high school graduation equations that include the instruments. The Davidson-MacKinnon test suggest that probits are not appropriate, especially when the endogeneity of mobility and homeownership are jointly tested (the even columns). As for the relevance of the instruments, state homeownership rates appear to be highly correlated with individual homeownership (with a point estimate of roughly 1.0 and a standard error of 0.05) in the full sample. The F-statistic of the state homeownership instrument, over 200 for the full sample, indicates that we should reject the hypothesis that the instrument coefficients are equal to zero. This easily exceeds the minimum F-statistic standard of 10 set by Staiger and Stock [12]. The partial R^2 for this instrument is approximately 0.05 to 0.06. The pre-school mobility measure also easily exceed minimum F-statistic and partial R^2 thresholds. These test statistics suggest that the instruments may be useful for identification purposes.

mobility, this effect is not statistically different from zero. The lone exception is the low mobility neighborhood sample. When including household mobility controls, a statistically significant 5.7 (1.6) percent homeownership effect remains. Therefore, the evidence suggests that homeownership endogeneity may be important, especially in higher turnover communities. But given the strong impact of residential mobility, even after attempts to correct for endogeneity, homeownership as a means of reducing community turnover may still be important to children's educational outcomes.[7]

Conclusions

This brief note adds to recent work that attempts to identify homeownership externalities. The results suggest that some of the homeownership effect found in Green and White [5] is driven by family characteristics associated with homeownership, especially residential stability. As much as homeownership increases residential stability, it appears to be correlated with higher school attainment. Attempts to control for endogeneity, however imperfect, cannot eliminate this finding. This work compliments the findings in Hanushek, Kain, and Rivkin [6] that document losses in achievement in early grades from school mobility. Nevertheless, it must be kept in mind that there are costs to housing investment that may counterbalance any positive externalities from housing investment. Most importantly, time and money committed to neighborhood capital and housing investment might be offset by reduced input into family-specific investments that have a more direct payoff to children's outcomes. Therefore, future research should attempt to understand why it is that this correlation exists and whether it can justify policy attempts to subsidize housing.

[7] An alternative method to control for family-specific unobservables is to identify the homeownership effect using sibling data. Conditional fixed effect logit models allow identification of within-family variation in homeownership status. However, caution must be used in interpreting the results since other family changes over time that affect the educational success of the siblings differentially may be correlated with switches in homeownership. I use homeownership variables measured at four different ages – 10, 12, 14 ,and 16. In three of the four cases, the homeownership coefficient is significant and the same magnitude as OLS when no mobility controls are included. Once mobility is added, none of the homeownership coefficients are statistically different from zero.

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Table 1
The effect of adding duration and mobility measures to the homeownership effect 1
Partial derivatives (standard error in parentheses)

Dependent variable: whether child graduated from high school by age 19

	(1)	(2)	(3)	(4)	(5)	(6)	
homeowner, age 7-16	0.096	0.054	0.054	0.055	0.052	0.046	0.048
	(0.015)	(0.016)	(0.017)	(0.017)	(0.016)	(0.018)	(0.016)
max. duration in residence, 7-16					0.002	0.002	0.002
					(0.001)	(0.001)	(0.001)
% years moved, 7-16		-0.214	-0.223	-0.257	-0.175		
		(0.029)	(0.038)	(0.032)	(0.034)		
% years changed zip code, 7-16			-0.011				
			(0.045)				
% years changed state, 7-16				0.207			
				(0.086)			
% years moved as homeowner						-0.125	
						(0.060)	
% years moved as renter						-0.216	
						(0.044)	
% years job moves, 7-16							-0.127
							(0.059)
% years consumption moves, 7-16							-0.208
							(0.053)
% years neighborhood moves, 7-16							-0.227

							(0.164)
% years other/mixed moves, 7-16							-0.266
							(0.069)
Sample size	5,143	5,143	4,926	4,926	5,143	5,143	5,143
Log likelihood	-2,740	-2,711	-2,598	-2,595	-2,707	-2,707	-2,702

Notes:
1 Regressions control for family income, dummy variables for year, race, gender, race*gender, step family, young birth (child born to parent under age 18), household head's education, number in family, female household headed family, household head's marital status, and household head's work status. All time varying control variables are averaged over age 7 to 16. The marginal effects are calculated for a male child in a 4 member white family in 1984 with two parents, average income,where the household head is continuously working and is a high school graduate. All mobility and duration variables are measured at the mean.

Table 2
The effect of adding other wealth measures to the homeownership effect

				Partial derivatives (standard error in parentheses) Dependent variable: whether child graduated from high school by age 19				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
homeowner, age 7-16	0.063	0.031	0.045	0.062	0.063	0.063	0.064	0.060
	(0.029)	(0.035)	(0.032)	(0.030)	(0.029)	(0.029)	(0.029)	(0.031)
max. duration in residence, 7-16	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
% years moved, 7-16	-0.170	-0.174	-0.176	-0.172	-0.169	-0.169	-0.169	-0.183
	(0.059)	(0.061)	(0.064)	(0.061)	(0.059)	(0.059)	(0.059)	(0.065)
house equity, age 7-16 /10,000		0.009						
		(0.006)						
house equity squared, age 7-16		-0.0004						
		(0.0004)						
home equity (from asset survey) / 10,000			0.012					
			(0.006)					
home equity squared			-0.0006					
			(0.0003)					
total assets / 100,000				0.010				
				(0.009)				
total assets squared				-0.0001				
				(0.0002)				
total assets less home equity / 100,000					0.012			
					(0.011)			
total assets less home equity squared					-0.0001			
					(0.0003)			
total debt / 10,000						0.002		
						(0.037)		
total debt squared						-0.004		
						(0.007)		
total other real estate / 10,000							-0.004	
							(0.004)	
total other real estate squared							0.0001	

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
							(0.0001)	
total vehicle assets / 10,000								0.041
								(0.019)
total vehicle assets squared								-0.003
								(0.002)
Log likelihood	-1,068	-1,066	-1,065	-1,067	-1,066	-1,067	-1,067	-1,065

Notes: See table 1. Asset data comes from 1984 and 1989 supplements. Kids who turn 17 during 1985 to 1989 are assigned the 1984 value. Kids who turn 17 between 1990 and 1994 are assigned the 1989 asset value. All asset values are in real terms. Sample size is 2,062 for all columns.

Table 3
Homeownership effects by neighborhood mobility
Partial derivatives (standard error in parentheses)

	High mobility neighborhoods			Low mobility neighborhoods		
	(1)	(2)	(3)	(4)	(5)	(6)
homeowner, age 7-16	0.062	0.025	0.025	0.140	0.091	0.090
	(0.020)	(0.021)	(0.023)	(0.026)	(0.027)	(0.027)
max. duration in residence, 7-16		0.0004	0.0007		0.0033	0.0032
		(0.0011)	(0.0014)		(0.0014)	(0.0014)
% years moved, 7-16		-0.090			-0.287	
		(0.028)			(0.063)	
% years moved as homeowner						

% years moved as renter						
% years job moves, 7-16			-0.059			-0.222
			(0.069)			(0.112)
% years consumption moves, 7-16			-0.149			-0.294
			(0.062)			(0.102)
% years neighborhood moves, 7-16			-0.205			-0.311
			(0.198)			(0.314)
% years other/mixed moves, 7-16			-0.188			-0.479
			(0.082)			(0.137)
Wald statistic of differences between samples:						
homeowner, age 7-16				5.9	3.8	3.3
max. duration in residence, 7-16					2.8	1.5
% years moved, 7-16					8.1	
Sample size	2,545	2,545	2,545	2,355	2,355	2,355
Log likelihood	-1,376	-1,367	-1,365	-1,211	-1,209	-1,182

Notes: See table 1. Low (high) mobility neighborhoods are defined as the bottom (top) half of the sample. The evaluation of partial effects use the means for the income and mobility variables separately for each stratified sample.

Table 4 Homeownership effects by neighborhood income level Partial derivatives (standard error in parentheses)

	Low income neighborhoods			High income neighborhoods		
	(1)	(2)	(3)	(4)	(5)	(6)
homeowner, age 7-16	0.120	0.074	0.067	0.042	0.009	0.005
	(0.038)	(0.040)	(0.040)	(0.024)	(0.026)	(0.024)
max. duration in residence, 7-16		0.004	0.004		0.001	0.001
		(0.002)	(0.002)		(0.001)	(0.001)
% years moved, 7-16		-0.238			-0.100	
		(0.094)			(0.053)	
% years moved as homeowner						
% years moved as renter						
% years job moves, 7-16			0.071			0.013
			(0.169)			(0.090)
% years consumption moves, 7-16			-0.396			-0.068
			(0.152)			(0.082)
% years neighborhood moves, 7-16			-0.621			-0.099
			(0.512)			(0.270)
% years other/mixed moves, 7-16			-0.399			-0.293
			(0.218)			(0.108)
Wald statistic of differences between samples:						
homeowner, age 7-16				3.1	1.9	1.8
max. duration in residence, 7-16					1.4	1.3
% years moved, 7-16					1.6	
Sample size	1,199	1,199	1,199	1,212	1,212	1,212
Log likelihood	-694	-685	-682	-494	-490	-487

Notes: See table 1. Low (high) income neighborhoods are defined as the bottom (top) half of the sample. The evaluation of partial effects use the means for the income and mobility variables separately for each stratified sample.

Table 5
Instrumental variables estimates1
Partial derivatives (standard error in parentheses)

Neighborhood sample:	Full	Full	Low income	Low income	High income	High income	Low mobility	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
homeowner, age 7-16	0.071	0.036	0.106	0.076	0.036	0.024	0.057	0.050
	(0.019)	(0.026)	(0.059)	(0.068)	(0.019)	(0.034)	(0.016)	(0.024)
% years moved, 7-16		-0.230		-0.366		-0.114		-0.190
		(0.050)		(0.142)		(0.053)		(0.043)
Wald statistic of differences between samples:								
homeowner, age 7-16					1.3	0.5		
% years moved, 7-16						2.8		
Sample size	2,481	2,481	531	531	620	620	1,181	1,181
Log likelihood	-1,265	-1,255	-299	-295	-238	-235	-571	-561

Partial R2 from instruments 2	0.053	0.060	0.076	0.075	0.088	0.085	0.060	0.061
		0.069		0.045		0.074		0.055
F-statistic of instruments 2	216.1	126.5	72.1	36.0	65.6	43.0	116.6	59.6
		107.9		15.7		31.0		41.6
Davidson-MacKinnon chi2 statistic	3.9	14.6	3.6	9.3	2.9	13.8	2.4	1.9
Instruments:								
state homeownership	x	x	x	x	x	x	x	
mobility age 0-5		x		x		x		

Notes:
1 See tables 1, 3 and 4. Instrumental variable estimates use homeownership rate by state-year-race-income quintile and household mobility at ages 0 to 5 as the identitying instruments.Standard errors are calculated using a bootstrap with 500 replications. Full sample includes neighborhood data which is available through 1985.
2 First row reports partial R2 and F-statistics from homeownership equation and second row reports partial R2 and F-statistics from mobility equation.